

Health Care Financing Grants and Contracts Reports

Effectiveness of Preventive
Child Health Care

PUBS
RJ
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1981

Published by the Health Care Financing Administration
Office of Research, Demonstrations, and Statistics

Health Care Financing Grants and Contracts Reports

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Health Care

Financing Grants and Contracts Report

Effectiveness of Preventive Child Health Care

This report was made pursuant to HCFA Grant No. 18-97265/5-01 between the Office of Research, Demonstrations, and Statistics/ Health Care Financing Administration and the Northwestern University, Center for Health Services and Policy Research.

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Published by DHHS
Health Care Financing Administration
Office of Research, Demonstrations,
and Statistics
April 1981

EXECUTIVE SUMMARY

There is growing controversy concerning the effectiveness of preventive child health care. Some authors suggest that it has little impact on children's health, while others feel that children need continuous preventive care. Much of this controversy seems loosely, if at all, based in the empirical literature. This paper examined policy statements and the empirical literature to provide a more data based analysis of this important question.

Much of the discussion and controversy concerning preventive child health care occurs in the policy arena. A number of authors have recently criticized such preventive activities as screening tests, physical examinations, comprehensive care, and in some cases, nearly all preventive care. These authors suggest that such care affects neither access to services, nor children's health, and that it is not cost-effective.

However, close examination of statements that are made largely for a policy-making audience reveals that such papers suffer from a number of major flaws which can fail to do justice to the empirical data that does exist about preventive care. This is particularly evident since, till the recent Canadian Task Force Report, there were no systematic, methodologically critical reviews of the empirical literature concerning any aspect of prevention. Examination of policy discussions suggest that they suffer from the following flaws: 1) Citations and references are not systematically gathered and presented, but are used selectively to make a point; 2) Few references are cited; 3) Interpretations of research are often done so as to be consistent with the case being made, even if it does not adequately present the original research; and 4) Contradictory evidence is rarely cited.

In view of this, a review of almost 150 books and articles was conducted which identified 38 controlled studies of the effectiveness of preventive child health care. Studies were reviewed in such areas as 1) comprehensive care, 2) dental care, 3) health education 4) iron deficiency anemia, 5) lead poisoning, 6) otitis media, 7) tonsillectomy/adenoidectomy, 8) mental health, 9) screening tests, and 10) physical examinations. Overall, the bulk of the evidence suggests that preventive child health care does have a beneficial effect. At least 15 studies found clear positive effect; 19 studies found mixed effect; and only 4 studies found no effect at all.

Though these findings were fairly consistent across 8 of the 10 areas, for both comprehensive care and for physical examinations, there was less clear positive effect. Both of these are broad, complex, and multi-faceted interventions, the kind that are notoriously difficult to evaluate compared to specific interventions aimed at specific problems.

The results also suggested that when preventive efforts fail, it may largely be due to a failure to completely administer the treatment. In complex interventions, it is easy to overlook something, and potential problems can be missed at a number of places. Other times when prevention fails, it is because the patient simply fails to follow orders. Research should be directed towards ameliorating these two problems.

Since the experimental methodologies used in the 38 empirical studies were of mediocre quality in general, it is difficult to place much weight on any conclusions drawn from this examination. Nonetheless, it does not appear that the positive effects of prevention found in this study could be explained away on the basis of poor methods. It is probably fairest to say that preventive measures have, in general, been given only the weakest of tests, but that those tests are somewhat encouraging. Prevention still seems a reasonably good idea.

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Effectiveness of Preventive Child Health Care

"Children need continuing and comprehensive medical
attention aimed at prevention"
(Harvard Child Health Project, 1977)

"Pediatric care has little impact on children's health"
(Ghez & Grossman, 1979)

These statements are endpoints of a continuum of opinions concerning preventive child health care. There seems to be an increasing polarization of viewpoints on this issue in the literature. Some authors suggest that such preventive care is effective and should be central to a comprehensive approach to child health. Such persons generally are favorable to efforts to increase the health services to children, including federally mandated programs like Early and Periodic Screening, Diagnosis and Treatment (EPSDT) and the Child Health Assurance Program (CHAP). Other authors suggest that such efforts are not worth the time or money. They are cautious in recommending increased health services carte blanche, preferring to see more and better evidence that such services are worthwhile.

The issue is not as lucidly stated or clearly framed in the literature as might be thought at first reading. The object of criticism is unclear, shifting from treatment to diagnosis to screening to physical examinations. The criteria for effectiveness include improved distribution of health services, improved health status in children, and demonstrated financial benefits to society. Some authors are remarkably clear in their critiques; others fail to address basic definitional issues. Thus, what could be a direct and candid exchange of viewpoints and evidence is often a confusing morass of unfocused opinion.

A major factor in this confusion is poor reliance on relevant empirical literature. A central thesis of the present article is that views are proliferating among both advocates and critics in spite of the evidence rather than because of it. There are few sound analysis of the empirical literature, but a plethora of poorly grounded opinion. In the present paper, both opinion and evidence will be presented with the goal of abandoning poorly grounded conclusions and identifying more valid and productive ones for further research.

Terminology

Many terms are used in the preventive health care literature, and some conventions are common regarding their usage. One distinction is between efficacy and effectiveness. To quote Starfield (1977a):

Efficacy denotes the degree to which diagnostic and therapeutic procedures used in practice can be supported by scientific evidence of their usefulness under optimum conditions. Whether or not these procedures are applied adequately in practice, and whether they produce the intended results when so applied are matters of effectiveness. (p. 71).

Efficacy is necessary to, but not sufficient for, effectiveness. This paper focuses on effectiveness. It is often of most concern to policymakers, who accept the efficacy of procedure under ideal conditions, but have to deal with less than ideal conditions in implementing it.

Prevention is classically defined as either primary or secondary:

Primary prevention is defined as those measures designed to prevent a health problem, either acute or chronic, from occurring....

Secondary prevention consists of preventing or mitigating the effects of a health problem. Secondary prevention begins with early recognition of acute illnesses or conditions which may cause or result in sequelae which are disabling (Wallace & Oglesby, 1972, pp. 554-555).

This description sounds more precise than it is. Wallace and Oglesby list 43 categories of health services that are aspects of preventive maternal and child health care. They include family planning, poison control programs, immunization, treatment of otitis media, and mass screening programs. In fact, this diversity is apparent whenever authors attempt to define prevention (e.g. Mechanic, 1978). Schweitzer thus notes:

The term "preventive medicine" does not represent a single act of diagnosis and treatment; rather it represents a bundle of services that have to be analyzed both singly and jointly to account for differing diagnostic and treatment parameters. (p. 23).

The present paper will examine a wide variety of activities that fall under the preventive rubric. This is not to suggest that more circumscribed studies should not be undertaken. In fact, they would have the distinct advantage of allowing more concerted attention to the fine details that is needed to draw the most valid conclusions.

Overview of the Issues

In order to identify the issues more clearly, it is useful to examine in more detail the criticisms of preventive child health care.

Preventive Child Health Care

The concept of preventive child health care covers a wide range of activities, and some authors have chosen to make broad criticisms. Ghez and Grossman (1979), for example, say:

In a sense it is not suprising that pediatric care has little impact on children's health. Many of their health problems are either self-limiting, such as morbidity from acute conditions, or irreversible, such as congenital abnormalities of the neurological system. But the studies reviewed above indicate that this lack of potency extends to health problems that are capable of being affected by pediatric care and by family decisions concerning diet, and other forms of at-home health care, as modified by the advice of physicians. (p. 19).

Certainly this is a sweeping criticism. These authors, however, do acknowledge benefits from dental care, for prenatal care, and for infant care to age one. Indeed, there are few if any major criticisms of either dental or prenatal care in the literature.

Mechanic (1978) gives a more limited criticism:

Early detection of vision and hearing difficulties also limits later problems and costs and does not involve major behavioral barriers. Appropriate treatment of common childhood ailments such as streptococcal infections and otitis media avoid damaging

secondary problems that may result in the consumption of considerable services in adult life. The difficulty is that beyond a limited number of instances, preventive or early care remains an untested concept, and the costs of identifying a small number of cases of asymptomatic illness early may be prohibitive. (p. 250).

This perspective will prove more cogent in view of the literature. One might, however, elaborate his comment about prevention being untested. Even the "few instances" he notes provide rather poor tests.

Child health care is not the only area that has been criticized as ineffective. Marmor (1977a) and Schweitzer (1974) have attacked adult care. Marmor (1977b) says:

It should be noted that there is little evidence to support the general enthusiasm for preventive care. There are indications that prenatal care effectively prevents some maternal and infant problems. But there is also evidence that mass screening programs and even annual physical checkups are wasteful, and only occasionally detect conditions that are aided by early treatment. Economists Burton Weisbrod and Ralph Andreano conclude that preventive care can increase costs without significantly raising the level of health. They attribute apparent cost-savings in the Kaiser-Permanente plan (which is often cited as a model of the medical financial efficacy of prevention) to "various factors, many of which are unrelated to preventive care". (p. 84).

It is unfortunate that both child and adult issues have been treated together in this quote. The implication would seem to be that child prevention is unsupported, though it is unclear that Marmor actually intended that meaning. What may be true with adults may not be true for children.

Screening is a specific preventive activity related to early detection of people at risk for disease. In general, it consists of routine tests, easily administered on a mass basis, to identify those who might be at risk for a health problem. It has received extensive attention in the literature. Some authors have suggested that with, few or no exceptions, screening procedures cannot satisfy any of a number of criteria that might justify their use. (Bernick, 1977a, 1977b; Holt, 1974; White, 1975). These criteria include increases in the patients' quality of life, focus on potentially serious diseases, availability of accurate screening tests, potential for treating the disease, and economic benefit to society, among others (Bailey et al, 1974; Eggertsen et al, 1980; Frankenburg, 1973). Holland notes that "screening in childhood, notably in the area of development, has not yet undergone the careful scrutiny in merits", (p. 1495).

Finally, a few authors have suggested that routine physical examinations (a comprehensive examination, often by a physician or nurse practitioner, of a person's health history and physical systems) are of dubious worth (Braren & Elinson, 1972; Yankauer and colleagues, 1955, 1956, 1957, 1962). Thus Yankauer and Lawrence (1955) suggest that a "school medical examination of first-grade children who had previously been examined in kindergarten was valueless from a case-finding standpoint" (p. 78).

Outcome Measures

A final point is to note that several conceptually different types of outcome variables that have been used in evaluating child health care. They

fall into three categories, distribution of and access to health services, cost/benefit, and health status.

Some authors argue forcefully that equity in the distribution of health services should be a clear and paramount goal for national child health policy (Schorr, 1978). This goal stems from observations of existing inequities in distribution. Certain groups (e.g., the poor, certain minorities) had less access to health services, or access to substantially different kinds of services (e.g., emergency room) than other constituencies in this country (German^e et al, 1976).

Other authors suggest use of cost-benefit or cost-effectiveness data in evaluating preventive care (Bay, Flathman, & Nestman, 1976; Schweitzer, 1974). They suggest that unless a given procedure yields demonstrable financial gains to either the person, some institution, or to society, it should not be used. This criterion has been used infrequently, probably due to the difficulty of doing true cost-benefit analyses (Lave & Lave, 1978).

The most frequent criterion for evaluating preventive health services is health status. Measures of this variable are as broad as mortality and as specific as morbidity indices of specific conditions.

It is difficult, however, to argue that one of these three kinds of variables is more important than another. The choice of an outcome variable is at least in part a value judgement and a political act, and in part a statement of the practical options open to the researcher.

Prevention and Empirical Literature

Two general sets of observations support the contention that much has been written about preventive child health care that is not well grounded in empirical literature. First, there are no methodologically critical reviews of literature directly concerned with preventive child health care. The recent report of the Canadian Medical Association Task Force on the Periodic Health Examination (1979) is relevant to the present topic. It does assess the quality of the experimental design, and its findings are consistent with the conclusions that will be reached in this paper. Other reviews do not attempt to address any aspect of methodological quality. Therefore, the credibility of findings in light of methods will be the major focus of the present paper in a later section.

Second, many statements that have been made regarding preventive child health care may not have afforded the empirical literature adequate presentation. These statements frequently occur in the context of presentations of policy options. Thus, Marmor (1977a, 1977b) discusses prevention in the context of national health insurance, as do Ghez and Grossman (1979). The authors' purpose usually is not to provide any comprehensive review of the literature regarding an aspect of prevention (such as effectiveness). Rather, it is to support or oppose a particular policy. It is fair to note that these authors probably do not consider it their role to provide a scientifically sophisticated analysis of the evidence in support of their position. Rather, similar to legal tradition, citations and references are used selectively to make one's case. The results of such policy-oriented articles, however, do not always reflect the available empirical evidence. This is true in the present case.

This is not to suggest that empirical research should be the only factor to enter into a policy decision. The democratic process demands multiple inputs from multiple constituencies, of which evidence from the scientific community is only a small part (Cook & Cook, 1977). But that evidence should be given a fair presentation. An examination of a sample of policy statements will illustrate this point.

Policy statements make many points in limited space. Thorough presentation of references is usually sacrificed. Chez and Grossman present more references than most authors, yet they cite only nine references in support of their critique of pediatric care. Not all are empirical studies, and not all are cited in published form. While they acknowledge being selective rather than exhaustive, a more judicious choice of citations might have offered more support to their case. Schorr (1978), an avowed advocate of child health care, cites only two references in regards to routine child health services to support her broad conclusion that "the evidence is clear that personal health services are important and, when delivered properly, can contribute significantly to health and well-being" (p. 371).

Policy statements, as a form of advocacy, are prone to interpret things as consistent with the case being made, when possible. Consider again what Marmor says about preventive care (1977a):

Economists Burton Weisbrod and Ralph Andreano conclude that preventive care can increase costs without significantly raising the level of health. They attribute apparent cost-savings in the Kaiser-Permanente plan (which is often cited as a model of the medical and financial efficacy of prevention) to "various factors, many of which are unrelated to preventive care" (p. 84).

One is tempted to infer that Andreano and Weisbrod made these conclusions and attributions after an empirical analysis of either preventive care or of the Kaiser-Permanente plan. Reference to the original work (Andreano & Weisbrod, 1974) leaves a different impression:

Nevertheless, there is little if any evidence that either the organizational change or the emphasis on preventive care actually lowers cost. The apparent cost-saving in the Kaiser Permanente Groups has been attributed to various factors, many of which are unrelated to preventive care, and mass inoculation and screening programs have been attacked as being wasteful and uneconomic. Even the annual physical checkup has been criticized for consuming large amounts of physician time and detecting conditions that are aided by early treatment only occasionally.

Those authors then provide one reference in support of what they say. Andreano and Weisbrod have not analysed either preventive care or Kaiser Permanente. They do not make the attributions about Kaiser Permanente that Marmor states. Rather, they seem to be quoting from another source. Finally, they do not conclude that preventive care can increase costs, but that there is little evidence that it lowers cost.

As another example, Ghez and Grossman (1979) cite Kessner (1974) as an empirical study that allegedly supports the case that pediatric care is ineffective: "Kessner finds that source of care has no effect on prevalence of the three health conditions" (pp. 18-19). It is not clear what this statement has to do with the effectiveness of pediatric care itself. If source of care has no effect, it could be that all sources are equally effective or equally ineffective. Kessner's study, in fact, was not aimed at assessing preventive

care, but rather at assessing "the health status of a selected population,...differences in provider performance, organization, and attitude, and...information about the strengths and weaknesses of the tracer method." (p. xiv). Ironically, they note the possibility that the "data may well be used inappropriately to...draw very broad conclusions about the delivery of health services" (p. xv).

Finally, advocacy papers rarely cite contradicting evidence. Ghez and Grossman (1979), for example, report evidence of decreases in infant mortality in Mississippi, cited in a paragraph by Davis and Schoen (1978, p. 176), in support of a case that prenatal care is effective. They fail to report a case from the same book in the very next paragraph which some would say opposes their conclusion that pediatric care is ineffective.

There is tremendous pressure today to provide "policy relevant" answers to pressing questions. This can easily have a deleterious effect on the quality of any review, including the present one. Good work takes time. Lacking that time, one is often forced to use references which may be tangentially relevant but quickly available. One bases conclusions on equivocal evidence. All this occurs as part of an honest effort to provide quick and relevant answers to policymakers' questions about "what is known". Incomplete or inadequate representation of the empirical literature is the unfortunate but frequent result.

Review of Empirical Literature

A legitimate way to assess the effectiveness of preventive child health care is to examine empirical literature. What such a review may lack in scope and inclusiveness, it will gain in empirical validity. Knowledge of the

context of the literature review will help the reader put it in its proper perspective, as one of many legitimate contributions.

The author's academic training is in psychology and in methodology and evaluation research. This predisposes the review to include an analysis of methodology in a review of empirical studies. It is a basic premise of this paper that the credibility of the knowledge yielded by empirical studies cannot be understood fully outside of the context of methodology.

The review was conducted in a brief period of time. This will hamper conclusions in several ways. There inevitably will be literature that is directly relevant but was not obtained in time to be included. Moreover, the time available for synthesis of the literature was so limited that only relatively incomplete conclusions can be given.

The literature search was conducted as follows. Initially, a computer search of the literature was conducted using the Medlars and Dissertation Abstracts International databanks. Guiding concepts for construction of keywords were child, prevention, and effectiveness. These keywords were most directly relevant to the questions first asked. In retrospect, other words would have been useful, including screening, physical examination, and comprehensive care. Failure to use these words probably resulted in important, but unobtained, literature.

Then, copies of original sources were obtained when possible. Articles printed in foreign languages were not included. Time constraints would not permit locating or adequately translating them. Each article was abstracted, and relevant references from each article were recorded and obtained. This process resulted in 147 abstracted articles and books.

Of these articles, 38 receive particular attention. These 38 were selected using a number of criteria. The article had to address an activity

relevant to preventive child health efforts; this excluded a number of articles which dealt with a less relevant point such as adult prevention. The topic of the study had to assess the effectiveness of a procedure in a field setting, rather than efficacy in an ideal setting. The study had to report empirical data; this excluded opinion, advice to the practitioner, program descriptions, and reviews of the literature. Finally, each had to include a control condition of some kind, in addition to a treatment condition. Use of these criteria excluded many articles that reviews with other criteria might find relevant (e.g., Agustin et al, 1973).

The 38 articles were abstracted further. The results of that process are presented in Table 1. The first column of the table lists the study by first author's name and date of publication. The second column categorizes the experimental design according to the following system:

- I. Random assignment of subjects to control or experimental condition
- II. Study used separate experimental and control groups, but method of assignment to groups was either unspecified or nonrandom.
- III. Study used some sort of within subjects control condition, e.g., time series data, or a longitudinal study.

The third column lists the independent variables under study, and the fourth column presents the results for each dependent variable the author(s) reported.

A number of methods that have been proposed to synthesize literature. Meta-analysis (Glass, 1976) for example, allows assessing the magnitude of effect in numerical terms over a number of studies. This technique, while preferable in a number of other instances, is of limited utility in the present case due to the relatively small number of studies available and the poor reporting of statistics in the articles.

Table 1

38 Empirical Studies

Author	Design	Independent Variable	Results
<u>A. Comprehensive Care</u>			
Alpert, et al., (1968)	I	Comprehensive care program versus the usual medical care	As compared to the controls, the experimental group had fewer hospitalizations, operations and illness visits and more health visits.
Gordis (1973)	II	Census tracts in Baltimore with a comprehensive care program versus tracts without such programs.	Incidence of rheumatic fever decreased over time in experimental tracts to level below control tracts
Gordis and Markowitz (1971)	I	1) Comprehensive versus regular care, in infants from birth to one year	1) No effect on immunization levels, use of medication, mortality or morbidity.
	I	2) Continuous care versus traditional care over 15 months, in children with history of rheumatic fever	2) No effect on compliance with physicians' recommendations, as determined by periodic urine tests for penicillin.
Kaplan et al., (1972)	II	Health care project versus traditional care	The project produced an improvement in school attendance, but a small effect compared to other factors.
Klein et al., (1973)	II	Neighborhood health care center users versus nonusers versus comparison sample.	During the second year of operation, admission rate of health center users was only 33 versus 67 per thousand for the comparison group. Comparing first two years, hospital days per thousand dropped by half for users. The author concludes that the center did not affect child admissions, but noted users behaved differently than nonusers on the dependent variables.

Table 1, Continued

Author	Design	Independent Variable	Results
Leodolter (1978)	III	Austria's Mother-Child Health Passport program	Over several years, Austria's infant mortality, perinatal mortality, and maternal mortality all decreased while the program was in effect
Moore and Frank (1973)	III	Comprehensive health services to children	No relationship between levels of use of services and schools absenteeism.
Morehead et al., (1971)	II	OEO Neighborhood health centers versus four other types of health services providers.	OEO centers did generally well on an adequacy of care constructed from chart reviews, including child care.
Vaughn (1968)	II	Comprehensive care project for mothers and infants versus Dade county total versus women with no prenatal care	Project statistics indicated lower maternal mortality, stillbirth, neonatal mortality and premature births for comparison groups, particularly for women with no prenatal care.
<u>B. Dental Care</u>			
Brown (1975)	III	Comprehensive home and office dental care	Dental caries decreased over time; dental cleanliness increased; gingival health was unchanged. Some data to suggest that such preventive efforts are cost-effective.
Kurtzmann et al., (1974)	III	Mobile dental clinic in underserved area.	57.4% of children treated had never visited a dentist, providing some evidence for positive effect on distribution of care. Authors note greatly improved dental status (by priority ratings) over time.
Muhler (1968)	I	Six separate studies comparing controls with some combination of home and/or office dentifrice and care.	In all six studies, the effectiveness of such care was supported by reduction in number of dental caries.

Table 1, Continued

Author	Design	Independent Variable	Results
<u>C. Health Education</u>			
Biener (1975)	II	36 health education lectures to adolescent males	Experimental subjects exceeded controls in lung and respiratory functioning, physical strength, dental health, amount of exercise; they also smoked and drank less than controls.
Ezell (1975)	II	American Dental Association dental health program versus traditional dental health program	Both programs increased oral health attitudes and behaviors; the ADA program increased oral health behaviors more than the traditional program.
Williams (1975)	II	Preventive dental health education program versus no program.	Plaque accumulation decreased in program participants.
<u>D. Iron Deficiency Anemia</u>			
Brigety & Pearson (1970)	II	Dietary supplementation of iron versus diet plus medicinal supplementation.	One third of all subjects hemotocrit values increased, while two thirds remained the same. No differences between experimental and control groups.
Crawford (1970)	III	Liquid ferric iron coupled with low weight carbohydrate (Niferex) to treat 52 anemic children	Hematocrit increased in 47 of 52 children
Starfield & Scheff (1972)	III	Medical care for anemia as determined by interview and chart review.	53 low hemogloin values found. 24 abnormal values not recognized. Of 29 recognized abnormalities, only 23 were diagnosed. Only 14 of the 53 were recognized, diagnosed, treated and reassessed.

Table 1, Continued

Author	Design	Independent Variable	Results
<u>E. Lead Poisoning</u>			
Byers (1959)	III	Intravenous or intramuscular edathamil calcium disodium to treat identified cases of increased lead burden.	Half of the patients returned to normal on tests of psychological function; but half did not, particularly if re-exposed to lead.
Pueschel et al., (1972)	III	Screen and treat for lead in a high risk area; follow up 1 1/2 years later.	8% of children had high lead burden. Of those found 1 1/2 years later, 75% had changed environmental contributing factors. Of those found and treated after initial screen, they had increased IQ and Sequin Formboard tests, but no change in Bender Gestalt test.
<u>F. Otitis Media</u>			
Ensign et al., (1960)	II	Sulfamethoxypyridazine (Kynex) to prevent otitis media in Indian children.	None of the study group who took the medication regularly had earaches or draining ears, but 12.6 per cent of the control group without previous draining ears had otitis media. Less success in children with destroyed or mutilated eardrums. Successful in children with previous draining ears.
Maynard et al., (1972)	II	Ampicillin prophylaxis for otitis media in Alaskan Eskimo children	Incidence of otitis media less in experimental group than in matched control. No difference in acute respiratory disorders.
Perrin et al.,	I	Sufisoxazole versus placebo as prophylaxis for otitis media	Experimental condition significantly decreased otitis media compared to placebo, for suburban children with recurrent history.

Table 1, Continued

Author	Design	Independent Variable	Results
<u>G. Tonsillectomy/Adenoidectomy</u>			
Mawson et al., (1968)	I	adenotonsillectomy in children	For dependent variables of catarrh, mouth breathing, snoring, coughing, both experimental and control groups decreased over time, but experimental group decreased more.
McKee (1963)	I	Tonsillectomy and adenoidectomy	Respiratory illnesses decreased in experimental group but not control. Of non-respiratory illnesses, only otitis media decreased in experimental group.
Royhouse (1970)	II & I for three total groups	Adenotonsillectomy	Principal benefits is reduction in throat disease. Increased in cough and chest infection for operated children as well as decrease in severity of colds and increase in gastrointestinal problems.
<u>H. Mental Health</u>			
Runquist (1976)	II	Once a week mental health consultation to teachers for problems with their students.	No change in teachers' attitudes to children nor in child's deviant behavior. Increases in child's socialized behavior and in parent/teacher contact. Decrease in number of outside referrals of problem children.
<u>I. Screening</u>			
Currier (1977)	III	EPSDT screen and referral	A drop of 13% occurred in the referral rate for health problems during a 6-month period among those who were screened a second time or more.
Department of Public Health, Portsmouth, Virginia	II	Screening and counseling versus screening versus no treatment controls	Controls exceeded experimentals in number of physician visits and prescriptions, number of hospital visits and hospital days, costs of physician visits and prescriptions, and total health costs.

Table 1, Continued

Author	Design	Independent Variable	Results
MacCready (1974)	III	PKU screening	After program had been in effect for several years, PKU admissions dropped to zero.
Spencer (1974)	III	PKU screens	After introduction of screens, no new PKU admissions between 1968 and 1974.
Webb et al.,	III	PKU screening	PKU admissions for retardation decreased to zero for children born after program started. Says costs of screen less than estimated cost of treating PKU after-math.

J. Physical Examinations

Anderson (1970)	III	A sample of routine physical examinations by pediatricians in private practice of infants in first year of life.	1.9% yielded significant abnormalities over the year. 3.9% yielded significant abnormalities at their initial exam.
Braren & Elinson (1972)	II	Single physical exam (stratified by age, one grouping being 0-19 years)	Exam did not seem to affect mortality assessed nine years later
Yankauer & Lawrence (1955)	III	Physical exam of 1st graders who had exam in kindergarten. Excludes conditions which could be identified by non-physicians.	5.5% had not had exam in last year. 21% had adverse condition. 2% had adverse condition neither known or treated. Only 1 child had serious condition requiring prompt attention
Yankauer & Lawrence (1956)	III	As above, with annual physical exams through fourth grade.	14% developed adverse conditions in 3 years. 38% of these were neither known nor under care. Only 1% had conditions that could not have been discovered by other means than a physician's examination.

Table 1, Continued

Author	Design	Independent Variable	Results
Yankauer et al., (1957)	III	As above	2% had severe or moderate handicap that could be completely or partially treated, but were not under care. Most adverse conditions were either irremediable, only slightly handicapping or already under care.
Yankauer et al., (1962)	III	Physical exam of first graders who were supposed to have exam within last year.	21% had adverse condition, 3% of which were neither known nor under care.

In view of this, the present review resorts to examining the results of each study in narrative form. This has its flaws, including failure to address the magnitude of the effect. Nonetheless, it presents the evidence for conclusions for all to see. While 38 studies is a small sample, they hopefully represent a fair sample of the literature. In the final analysis, however, the present review is only as conclusive as the 38 studies are representative.

Overall Results

The value of preventive child health care has been questioned broadly by authors (e.g., Ghez and Grossman, 1979). It is not clear that it is fair to ask such a broad question, however, in view of the many varied activities that fall under the rubric of prevention. Nonetheless, since a broad challenge has been put by some, it is justified to examine the literature in Table 1 as a whole to provide a broad answer.

The answer is clear. The bulk of the evidence suggests that prevention does have a beneficial effect on the dependent variables reported by the authors. Only four studies could reasonably be interpreted to suggest that it has no effect (Braren & Elinson, 1972; Gordis & Markowitz, 1971; Klein et al, 1973; Moore & Frank, 1973). On the other hand, at least 15 studies could be interpreted as suggesting that prevention has an unambiguous positive effect. (Alpert et al, 1968; Beiner, 1975; Brown, 1975; Crawford, 1970; Department of Public Health; Gordis, 1973; Kurtzmann et al, 1974; Leodolter, 1978; MacCready, 1974; Muhler, 1968; Perrin et al, 1974; Spencer, 1974; Vaughn, 1968; Webb et al, 1973; Williams, 1975). Positive effect is defined as unambiguous change on the only measure reported, or beneficial change on most or all measures reported. Admittedly, such attempts to classify

magnitude of effects are crude and judgmental at best. However, the fact that the number of strong positive effects so greatly outnumbered the no effect findings would suggest that were the judgements made by other people with other means, the results might still have favored positive conclusions.

The remaining 19 studies fall somewhere in between. At one extreme are studies like that by Yankauer and colleagues, which suggest a very minimal benefit, if any. At the other extreme are studies which report quite a few benefits, but also fail to find benefits on a number of other variables (e.g., McKee, 1963; Mawson et al, 1968). What unifies the twenty is that they all show at least some benefit. Every attempt was made to classify studies conservatively, not assigning a study to a category implying more benefit unless the reported evidence is strong. This conservative bias was deliberate so as to avoid the probability of finding a positive effect unless the evidence clearly supported it.

Prevention then, has at least a small positive effect, and perhaps a large one, with children. However, such a view is both cursory and not very critical. It is, therefore, enlightening to examine the studies more critically and in more detail. To do so, the studies will be discussed in the context of different types of preventive activities in the section immediately following. Then, the credibility of the evidence will be examined in a section following that.

Comprehensive Care

Nine studies assessed the impact of comprehensive care projects in preventing child health problems (Alpert et al, 1968; Gordis, 1973; Gordis and Markowitz, 1971; Kaplan et al, 1972; Klein et al, 1973; Leodolter, 1978; Moore & Frank, 1973; Morehead et al, 1971; Vaughn, 1968). These studies provide

some of the least consistent results of any specific category. Four suggest unambiguous benefits, two find no benefit, and three report some benefit.

By their very nature, comprehensive care programs subsume a large number of varied activities. It was rare for an author to present a fully detailed analysis of the care actually provided to the experimental group. Thus, it is entirely possible that the actual treatments differed considerably from study to study, which might lead to widely differing results across studies. Furthermore, the nature of the control conditions was usually simply described as traditional medical care, or regular care. As with the treatment conditions, the control conditions could also have varied widely in terms of treatment actually administered. It would seem that a necessary step to clarifying results would be a more detailed analysis of the treatment and control conditions.

It is not always clear how appropriate the dependent variable was to the treatment. Two studies finding little or no effect used school attendance as an outcome measure. As one of those studies suggests (Kaplan et al, 1972), so many factors besides use of health services determine school attendance that it may not be sensitive to the treatment.

In a similar vein, the Gordis (1973) study used incidence of rheumatic fever as an outcome variable. In fact, the author noted that this was done, in part, because "the comprehensive care programs in Baltimore have previously been shown to be heavy utilizerg of throat cultures" (p. 331). It would appear, then, that the construct being investigated was not really comprehensive care, but care specific to the treatment of rheumatic fever. One might be justified in suggesting that the data from this study support a conclusion that programs which heavily use throat cultures probably do so from

a specific concern to prevent or treat rheumatic fever, and that this specificity of effort produced benefits.

It is particularly interesting to compare two studies (Alpert et al, 1968; Gordis & Markowitz, 1971). Both used random assignment of subjects to conditions, and it appears that both used similar independent and dependent variables. Yet they arrived at strongly opposing conclusions. The Alpert study suggested comprehensive care of children does work. The other study suggests it does not work with first-born infants. One might appeal to the following factors to explain the difference. It could be that parents attended more conscientiously to the care of their first-born infants, thus suggesting that infants in both conditions received equally good and thorough care (a ceiling effect).

In both studies there was considerable attrition from both conditions. Without knowing the specific circumstances of the studies, it is difficult to use these observations definitively. However, differing reasons for attrition between the studies could have produced differing responses to treatment in the remaining subjects.

Finally, while similar dependent variables were used, the analyses differed in an important respect. The Alpert study analysed data at consecutive six month intervals over three years. The Gordis study examined the data for the first year of life. A closer examination of Tables 1 and 2 in the Alpert study would suggest that had the authors collapsed the data over the first year, the results would have been closer to the Gordis et al study. For example, hospitalizations for the comprehensive care group in the Alpert study actually were higher than for the control group for the first six months, but less than the control for the second six months. Averaging over the whole year (which cannot be done using the reported data, without

knowledge of the sample sizes for each mean) would suggest a much less dramatic difference.

Since all but one of the studies used traditional care as a control condition, the studies provide meager basis for assessing the effects of comprehensive care as compared to no care at all. The exception (Vaughn, 1968) does suggest that no care at all leads to considerably poorer outcome than comprehensive or traditional care. Taken all together, it could easily be suggested that comprehensive care does no worse than, and perhaps better, than traditional care.

Dental Care

Three studies (Brown, 1975; Kurtzmann et al, 1974; Muhler, 1968) all suggest strong benefits from dental care. The Kurtzmann study treated a population with little previous access to dental care. In that sense, the nature of the sample made it likely that any intervention would show some positive effect. The other two studies provide more convincing evidence, however. In particular, the Muhler study reported six separate experiments, all using random assignment of subjects to conditions, all of which suggested that the treatments reduced the incidence of dental caries in children. These results are consistent with the conclusion reached by Andlaw (1978) in a review of the literature, although Andlaw does note that it is not clear that children are as efficient in their oral hygiene as is ideal for prevention of dental caries.

Health Education

All three relevant studies (Biener, 1975; Ezell, 1975; Williams, 1975) suggest benefits resulting from health education. The Ezell study finds only

some benefit; but the treatment, an American Dental Association program, seems very similar to the control, a traditional dental education program. The other two studies suggested very strong benefits, and compared a health education program to no program at all. Both the Ezell and Williams studies were concerned only with dental health, while the Beiner study was more comprehensive in scope.

Iron Deficiency Anemia

Again, the three relevant studies (Brigety & Pearson, 1970; Crawford, 1970; Starfield & Scheff, 1972) all suggest some positive benefit from treatment. The strongest effect occurred in a study (Crawford, 1970) in which the sample was composed of children diagnosed as anemic. This study showed improved hematocrit values in 47 of 52 children. The effect was weaker in the Brigety & Pearson study. Their sample contained children at risk for anemia, rather than diagnosed as anemic. Further, they did not use a no treatment condition, but compared diet to diet plus iron supplements. Both these factors would serve to reduce the magnitude of effect. Finally, Starfield & Scheff report a fascinating study documenting how low hemoglobin values are often not even noticed, much less diagnosed, treated and followed. A "failure" of prevention could thus be a failure to begin treatment rather than a failure of treatment. This highlights the distinction between efficacy and effectiveness made at the beginning of this paper. It seems clear that any of a number of factors can contribute to a failure of prevention in field settings for this disorder (Starfield, 1977).

Lead Poisoning

Two relevant studies (Byers, 1959; Pueschel et al, 1972) suggest at least some benefit. Byers found that treatment of diagnosed children resulted in half returning to normal. Half did not do so, particularly if reexposed to lead. The Pueschel study reports an extensive screening and treatment program. Screening identified 8% of the sample as having increased lead burden; these were treated with both chelation therapy and environmental interventions. A re-examination 1.5 years later found some increases in intellectual performance. In addition, 75% of the families involved had taken steps to remove the environmental hazard.

Both these studies have fairly weak designs which rely on the assumption that no treatment would produce more damage than was observed in treated subjects. This assumption seems warranted (Graef, 1977), and at any rate, it is not clear that there are many ethical means of assigning subjects to "no treatment" control conditions. On the other hand, it should be possible to institute other quasi-experimental controls. For example, compare two cities with similar incidences of lead poisoning, one of which then initiates some treatment program.

Otitis Media

Three studies examined chemoprophylaxis of otitis media (Ensign et al, 1960; Maynard et al, 1972; Perrin et al, 1974), and all found benefits. Perrin et al (1974) found decreased incidence in the treatment group for suburban children with a previous history of otitis media. The other two studies found decreased incidence due to treatment in populations that were at risk for the disorder. Both the Ensign and Maynard studies also showed

dramatic preventive effects if the children took the medication as prescribed. Again, this illustrates the point that prevention can succeed only when it is applied.

Both McKee (1963) and Roydhouse (1970) reported decreased incidence and/or severity of otitis media in children undergoing tonsillectomy and adenoidectomy. The results of these five studies are consistent with conclusions reached by other reviews of the literature (Starfield, 1977).

Tonsillectomy/Adenoidectomy

All three relevant studies (Mawson et al, 1968; McKee, 1963; Roydhouse, 1970) report at least some benefit from the operation. Mawson et al (1968) found some improvement in all subjects, but most improvement in children with the operation. McKee (1963) found benefits only in preventing respiratory illnesses and otitis media, and only in the experimental group. Roydhouse suggested that the principal benefit was reduction in throat disease, but noted some potentially negative side effects such as increases in cough and in chest infection following the operation. It should be noted, therefore, that even though some benefit for these procedures has been identified, this does not mean that all children should be submitted to them. It only means that it cannot be said that these procedures, as preventive measures, have no effect.

Mental Health

No particular effort was made to identify preventive studies in this area. However, one study found through the computer search suggested at least some benefits from once a week mental health consultations with teachers (Runquist, 1976). Children of teachers who received the consultation displayed more socialized behavior, and there were more parent/teacher

contacts and a decrease in the number of outside referrals for problem children. A review of the literature by the President's Commission on Mental Health (1978) supports the effectiveness of a number of mental health preventive efforts.

Screening

Five studies directly studied the effects of screening, and all suggested at least some benefit (Currier, 1977; Department of Public Health; MacCready, 1974; Spencer, 1974; Webb et al, 1973). The latter three references suggest the effectiveness of PKU screening; where it is instituted admissions for treatment of PKU drop to zero. The Currier study examined the effectiveness of EPSDT. It found a drop of 13 percent in referral rate for health problems during a six-month period among those who were screened a second time or more. The Department of Public Health study found that in census tracts in which either screening or screening and counseling were available, there were fewer physician visits and prescriptions, less use of hospitals, and lower costs for physician visits and prescriptions, and lower total costs.

Relevant evidence for the potential effectiveness of screening programs is provided by Reis et al (1979). This study is a metaevaluation of the results of 15 EPSDT demonstration and evaluation projects funded by the Health Care Financing Administration of HEW. While noting the extremely poor methodological quality of the demonstrations, the authors concluded that the EPSDT programs did improve access to health services and resulted in at least some benefit to the health of children. Finally, most reviewers endorse use of screening tests with children (Bailey et al, 1974; Eggertsen et al, 1980; Frankenburg, 1973), although some express reservations (Holt, 1974).

Physical Examinations

There are six articles which examine the effectiveness of physical examinations (Anderson, 1970; Braren & Elinson, 1972; Yankauer & Lawrence, 1955; Yankauer & Lawrence, 1956; Yankauer et al, 1957; Yankauer et al, 1962). The studies by Yankauer and colleagues, and the study by Anderson, suggest that few significant abnormalities are found by a physician during routine physical examinations. Further, Yankauer et al (1957) suggest that most of what is found is irremediable, only slightly handicapping, or already under care. It should be noted, however, that the Yankauer studies exclude any condition which could be found without the skills of a physician, such as vision and hearing disorders which could be screened. They also deal with children who for the most part had an examination within the last year, and then excluded any abnormality that had been previously identified. These would serve to decrease the chances of finding "significant" abnormalities.

Braren & Elinson (1972) assessed the benefits of a single physical examination on mortality nine years later. It is not surprising that no effect was found with children. The comparatively weak intervention easily might not effect mortality with a small sample of children, as most might be expected to live for the whole nine years. Elinson himself (1974) later noted the inadequacies of mortality as a sociomedical health indicator.

The Canadian Medical Association recently published a review of the literature concerning periodic health examinations. Their monograph contains nearly 1500 citations. An executive summary of that report (Canadian Medical Association, 1979) gives the principal findings. Briefly stated, that task force found sufficient empirical studies or medical opinion (where empirical studies were not available) to recommend selective examinations of infants and children on a number of problems to which they are particularly susceptible.

That review made a serious attempt to classify the quality of the evidence available, and the strength with which any recommendation could be made. The task force noted that the empirical studies were frequently both few in number and generally poor in quality. On the other hand, it is clear that they find sufficient merit in the periodic health examination to recommend it on a regular but selective basis with children.

Summary

The effectiveness of preventive child health care in each of 10 areas suggests that there is a clear beneficial effect in at least eight of them. More ambiguous findings dominated the comprehensive care area and the physical examination categories. Both of these are complex and multi-faceted interventions, whereas (with the possible exception of screening) the remaining eight categories deal with more narrowly defined interventions aimed at specific targets. It seems logical to assume that with more specific interventions, there is better opportunity to fully and consistently apply the treatment. For example, most would consider PKU screening a relatively simple and easy to apply procedure. Yet Webb et al (1973) report a failure to administer this treatment which resulted in two cases of retardation. Consider how much more complex the situation is with either comprehensive care or physical examinations. The large number of different activities that are subsumed by such terms may be differentially or incompletely administered, as evidenced by Starfield & Scheff's (1972) study, or the work of Kessner (1974). Without knowing how well each of these treatments is administered, it is difficult to evaluate their success or failure. But on logical grounds, there is reason to suggest that part of the ambiguity of findings may be due to failures of implementation of treatment for more complex interventions.

So, the evidence reported in 38 controlled empirical studies suggests that preventive child health care is generally effective. The effect remained present whether the dependent variable was health status, access, or cost, although far too few studies used the latter two variables to allow confident conclusions. This conclusion holds for the broad concept of prevention, and in regards to the majority of more specific activities subsumed under the rubric of prevention. It stands in marked contrast to the more pessimistic conclusions noted earlier (e.g., Ghez & Grossman, 1979), but is hopefully more data-based.

Credibility of the Studies

The preceeding review of the 38 studies was in many respects uncritical. The authors' reports of outcome were accepted at face value, in general, as accurately reflecting the conditions and results of the study. Yet a closer analysis of these studies suggests that this should not always be the case. In many cases, the author's conclusions are not very credible. The primary factor accounting for this lack of credibility is poor methodology.

Such factors as use of control groups, method of assignment to groups, and measurement, are crucial aspects of methodology. The form they take is crucial for the inferences that may be made (Campbell & Stanley, 1963). These methods are as important in health services research as in any other field of scientific endeavor (Davis & Schoen, 1978). The profound effects of failure to consider methodology in health services can be seen in the devastating critiques of California's Medi-Cal Copayment Experiment (1974). This is just as true with the 38 empirical studies.

Experimental Design

It is widely thought that random assignment of subjects to experimental and control conditions is the ideal design for assessing the impact of a social program (Campbell & Stanley, 1963; Cook & Campbell, 1979). This opinion is by no means shared by all methodologists (e.g., Stanford Evaluation Consortium, 1976). On the other hand, all should agree that no matter what design is used, a great deal of attention should be given to assessing the plausibility of conclusions in light of the design. This is commonly done by examining the plausibility of various "threats to validity", a term coined by Campbell & Stanley (1963) to describe competing explanations which are not ruled out by the design. While it is not the function of this paper to present a primer on design, it will be useful to examine a few of these threats in light of the 38 empirical studies to assess how well competing explanations could explain the data.

Only seven of the 38 studies used random assignment to conditions. Fourteen included a control group but did not use random assignment. The remainder used either a within-subjects design or a time series approach. It was particularly disheartening that less than half of the studies used any separate control group, must less random assignment. It was even more discouraging that very few authors discussed competing explanations to their conclusions about cause and effect.

A number of studies examined only one group of children, using the children as their own control (e.g., Currier, 1977; MacCreedy, 1974). This kind of design is subject to a number of uncontrolled rival hypotheses explaining the potential change on the dependent variable. The author must assess how plausible the rival hypothesis is. When one can show that these potential alternative explanations are not really plausible in the study's

circumstances, then causal inferences are strengthened even with this weak design. A few examples will illustrate this.

History is a rival explanation that suggests that some other change-producing event occurred in addition to the treatment. In the Currier (1977) evaluation of EPSDT in Michigan, it is reasonable to ask if other events occurred which might affect the health of children. Were other Federal or State programs instituted? Was there any effort on the part of local physicians' organizations to attend to the needs of children more concertedly? It is not a difficult task for the author to take a little time to think about these possibilities and report their plausibility in the article.

Maturation is a rival explanation which suggests that biological or psychological processes which systematically vary over time in a child are responsible for observed changes. Some have suggested, for example, that diseases occur less frequently as children age. Currier (1977) suggests that EPSDT caused the drop in referral rate at reinterviews. It is plausible to suggest that the children were simply older, and older children have fewer diseases. On the other hand, maturation is probably not a good rival hypothesis for the findings of the three PKU studies (e.g., MacCready, 1974). That is, it is not plausible, based on past experience with the disorder, to suggest that children would have gotten better over time without intervention.

There are a number of other threats to validity relevant to this design, and the interested reader should review Campbell & Stanley (1963). The point, however, is that health services researchers have the responsibility to use the best design possible, and to assess the plausibility of rival hypotheses in all cases.

A similar line of reasoning can be applied to the studies using a control group without random assignment. This design has been called the non-equivalent control group design (Cambell & Stanley, 1963). It controls for history and maturation, but is subject to a different set of threats to validity, including regression artifacts and a host of interactions between selection into groups and such variables as maturation. The Klein et al (1973) study will illustrate this point. They compared users with nonusers of a neighborhood health care center. Users select themselves into treatment, and nonusers select themselves into the control condition. One might assume that there are one or more underlying variables which control this self-selection process. It is plausible to suggest that persons who are more conscientious about their health will take the time and effort to go to treatment. This has been referred to as "creaming" (Cook & Campbell, 1979), and has been noted in educational evaluations for quite a while. If people who use the health center are more conscientious they will probably tend to be healthier than those who are less conscientious. This would account for Klein et al's observation that users were at lower risk than nonusers for hospitalization.

Very few authors who used this type of design took the time and effort to assess the plausibility of rival hypotheses. One exception is the report of the Department of Public Health in Portsmouth, Virginia. In a section about weaknesses of the study, they note that a number of these alternative explanations are plausible alternative explanations of the changes in health patterns and costs that they would like to attribute to screening. The authors should be congratulated for this honesty and rigor. It is unfortunate that more such attempts were not undertaken by other authors.

A detailed analysis of rival hypotheses is a time consuming affair. It also requires intimate knowledge of the experimental conditions. The original researcher is usually in the best position to do this analysis. Post-hoc analyses like the present one have the more difficult task of an outsider trying to determine from one article what is and is not plausible. Therefore, it is the original researcher who should take responsibility for the task. It is a matter of good scholarship and good science.

Overall, given the generally poor designs used in the 38 studies, it is reasonable to suggest that the benefits found in the studies may be, in part, artifacts of some other variables. A few examples have been given to illustrate this, and it seems many more could be suggested with diligent effort.

Attrition

Subjects who originally participate in the research drop out as time progresses. They do so for any of a number of reasons. Such attrition can be a serious problem in research. For example, when subjects have been randomized initially to conditions, attrition for differential reasons can destroy the comparability of the groups at posttest. It is important to assess what reasons account for attrition. Gordis & Markowitz (1971) report exactly 50% attrition in both experimental and control groups, yet made no attempt to account for this. It is unusual to find such precisely identical rates of attrition. More frequently one finds differential attrition rates (Alpert et al, 1968); these authors do note their intent to discern the influence of differential attrition upon the final results of the study. Such an exercise is important and useful.

In many ways, the attrition problem is similar to the self-selection problem previously described. In randomized studies, when subjects are not assigned to a desirable treatment, they may be disappointed or angry and drop out. Remaining control subjects may experience these same feelings of resentment, and we do not know what effect this may have on results. Control subjects might leave the study to obtain desirable medical care elsewhere, leaving control subjects who are less conscientious about their medical care and, as suggested previously, less healthy.

Attrition is also a problem in single group designs. In the Currier (1977) study, for example, nearly 80% of the original subjects did not return for reinterview. It seems plausible to suggest that the 20% that did return were again those who were more conscientious about their health, and again healthier. They would then have fewer referrals at rescreen because they were healthier, not because EPSDT had any effect.

At a minimum, authors should examine differences between remaining subjects on whatever variables are available and relevant, such as socio-economic status, education, and health attitudes. This was rarely done in any of the 38 studies. Strategies for dealing with attrition are presented in Riecken et al (1974).

Measurement

Boruch and Gomez (1977) have presented some of the problems that are posed for evaluations by poor measurement. Less reliable and less sensitive measures can lead to relatively insensitive experiments. None of the authors of the 38 studies presented empirical evidence of the reliability of the measures they used. We can guess that there is some reliability data available for some of the measures, like the intelligence tests reported by

Pueschel et al (1972). On the other hand, reliability of the scoring of the Bender Visual Motor Gestalt test used by Pueschel is frequently low; this would decrease the chance of finding a significant effect for this variable. It is not clear what data are available regarding the reliability of such variables as clinical diagnosis, hematocrit values, and hospitalization rate.

The validity of a measure also influences the results of an experiment. It may seem logical to point out that the more sensitive a measure is to a manipulation, the more likely it is to show change. We can wonder how sensitive to the treatment are such variables as school attendance to health project use, or mortality to a single physical examination administered nine years previously. In a broader context, we must examine how relevant the variable is as an indicator of the construct of interest. In this regards, the 38 studies usually have included reasonably direct indicators of health, the exception being the questionable validity of school attendance as a measure of health status. Finally, a number of the 38 studies used only one such indicator. A number of methodologists (e.g., Reicken et al, 1974) suggest using multiple indicators for each construct--advice well worth following to increase the sensitivity of an experiment.

On the whole, despite poor reporting of reliability and validity, the 38 articles do not seem to have significant problems with sensitivity, in that most reported some changes on the measures. A number of cases reporting no differences (e.g., Braren & Elinson, 1972; Kaplan et al, 1972) may be a function of insensitive or invalid measurement rather than ineffective treatment, however.

Statistics

All of the 38 studies gathered data and were thus faced with the task of choosing a method for analysis and reporting that analysis. All the studies report at least some descriptive statistics such as percentages. Fewer studies, however, report inferential statistics, such as chi-square tests, and t-tests, to assess the statistical significance of a observed difference. More importantly, when it was done, it was not always reported in an optimally useful manner. For example, it is not clear if the small number of statistically significant differences reported by Beiner (1975) represent all the tests he conducted, or are a small subset of the 156 items he noted were evaluated by a computer. This makes an important difference, as one would expect about 7-8 significant differences in 156 variables by chance alone.

To facilitate future reviews of the literature, reporting of statistics ought to include sample size significance (either dichotomous at the .05 level, or the exact level), the exact value of an inferential statistic, a relevant contingency table, or a correlation between treatment and outcome (Glass, 1977). The more detail is reported, the easier it is for future reviewers to determine the magnitude of effect using such techniques as meta-analysis.

Discussion

Without a more thorough analysis of the methodology of the 38 studies than is allowed under present time constraints, it is difficult to assess the credibility of their results. Nonetheless, some conclusions can be forwarded.

The methodological quality of the 38 studies is mediocre on the whole, and extremely poor in some cases. Given this, the lack of attention to assessing the plausibility of rival hypotheses is unfortunate. It would appear that there are some legitimate competing explanations for some of the

findings (e.g., the Currier study). Nonetheless, this rather cursory examination does not seem to suggest that all the significant findings could be explained away. In fact, it seems that some of the nonsignificant findings may themselves be an artifact of insensitive measurement. On the whole, then, the credibility of these findings is not high; on the other hand, there does not seem to be enough reason to suggest the studies are totally inconclusive. It is probably most accurate to say that preventive measures have, in general, been given only the weakest of tests, but that those tests are somewhat encouraging.

Conclusions

It is unfortunate that a more definitive statement cannot be made at the present time about the effectiveness of preventive child health care. However, it is quite possible to begin to accumulate more useful evidence. This section will outline some of the steps that should be considered in such a research program.

It is quite possible to do a more productive review of the existing literature. Given more time and resources, the number of relevant empirical studies could be increased. A more compelling review could be accomplished using the techniques of secondary analysis already widely used in educational evaluations (Boruch & Wortman, 1978). If the original data can be obtained from the authors, the reviewer would be in a position to analyse the data using a more uniform approach, and using a number of analytical models to explore the data. It would also be more possible to use the techniques of meta-analysis (Glass, 1977) to obtain estimates of the magnitude of effects. Either of these two approaches would yield quite useful information.

A number of authors (Davis & Schoen, 1978; Green, 1977; Sackett & Holland, 1975) have commented on the need to use more rigorous methodological standards in research in health services. To their comments is added the observation from this review that there is room for great improvement in the methodological quality of preventive child health research. In particular, it is most important to use the best design that is practical and ethical, and to analyze potential threats to validity in all cases.

A number of substantive issues can be forwarded as ripe for investigation. One of these is to begin to identify successful types of preventive care. This will require a broader perspective than a single study. Reviews of the literature, meta-analyses, and secondary analyses are ways of addressing the issue. A survey of existing preventive practices would also be useful. For example, EPSDT is not a uniform program that can be evaluated as a coherent whole. It is a national effort composed of a myriad of different projects. We need to know what those activities are, and to begin to establish a typology of them. One can then use a number of methods to get a first approximation to knowing which of these types is successful. Such methods include everything from asking key informants to pilot studies using simple indicators of success. Subsequently, one can do more detailed and sophisticated studies to explore successful projects.

There seems to be ample evidence to suggest that future research ought to examine the process of preventive care. It is clear that many times when it seems to fail, it is actually never delivered (Ensign et al, 1960; Hansen, 1975; Kessner, 1974; Lin-Fu, 1970; Starfield & Scheff, 1972; Webb et al, 1973). The challenge is to identify at what point the implementation of the treatment fails, and then to identify how that situation can be remedied. For example, Starfield & Scheff (1972) show that abnormal hemoglobin values are

often never recognized. It might be possible to highlight abnormal values so they will be noticed more easily. While it may not be possible to eliminate all failures to implement the treatment, some useful reduction is certainly attainable.

Sometimes when prevention fails, it may be because the patient simply fails to carry out the doctor's orders. Here, too, it is possible to learn through research. We need to form and study a model of preventive health behavior. There have been useful first steps already. Langlie (1976), for example, found empirical evidence for an active-passive dimension of preventive health behavior. The active dimension required an individual to perform an overt act, such as getting a checkup, or exercise. Passive health behaviors required that the individual abstain from certain acts, such as smoking. Langlie then found a number of interesting personality, demographic, and behavioral correlates of this dimension. The point is that by understanding the behavior of people in relation to prevention, we will be in a better position to influence it.

These, then, are some of the things that can be done to provide better and more complete evidence about the effectiveness of preventive child health care. They are in the spirit of the present literature review, in that they advocate a more data based approach to addressing issues in this area. At a minimum, it is hoped that this review has given a fairer presentation of the empirical literature than has been accomplished previously. More optimistically, it may provide the basis for both opponents and proponents to have a more rational dialogue concerning the intricacies of the issues involved.

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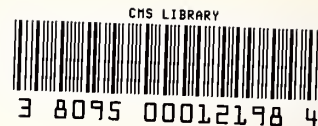
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